Some Remarks on the "Intelligent Design" Controversy

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1. Introduction: Separating Out Five Questions Concerning "Intelligent Design"

There are various questions that arise in connection with the "intelligent design" (ID) controversy. This introductory section aims to distinguish five of these questions. Later sections are devoted to detailed discussions of each of these five questions. The first (and central) question is the one that has been discussed most frequently in the news lately:

(**Q1**) Should ID be taught in our public schools?

It is helpful to break this general "public school curriculum question" into the following two more specific sub-questions:

(**Q1.1**) Should ID be included in the *science* curriculum of our public schools?

(**Q1.2**) Should ID be included in *some* part of our public school curriculum?

Of course, these public school curriculum questions should be distinguished from other (perhaps related, but distinct) questions that are often asked about ID. Here is another question that is very frequently discussed, not only in the debate about (Q1), but in the ID controversy generally:

(**Q2**) What *is* ID? In other words, is ID a scientific theory, a religious doctrine, an epistemological stance, or something else altogether? [Specifically, is ID a kind of creationism, or a kind of contemporary empirical science, or something else?]

We might call (Q2) the "demarcation question", because it requires *demarcating the scope* of ID. Much of the focus in the current debates surrounding ID has been on the demarcation question.[1] In this paper, one of my aims is to argue that this question (Q2) may not be as important to the ID controversy as people have seemed to assume. Another closely related aim of this paper is to shift the focus of the debate away from (Q2), and toward *epistemological* questions, such as:

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[1] This paper was largely inspired by the recent legal case *Kitzmiller et al. v. Dover Area School District et al.* See (Brauer *et al.*, 2005) for an in-depth discussion of the considerations that arise in the context of the *legal* (specifically, *constitutional*) rendition our central question (Q1). I am not so interested in such *legal* questions here. But, I will comment on both the socio-political and (much more briefly) the legal aspects of the ID controversy in the final section (for my remarks on the legal aspects of the current controversy, see the final footnote of this paper).

(**Q3**) Are there (*any*) phenomena (scientific evidence) that current evolutionary theory (ET) is unable to adequately predict and/or explain?

We might call (Q3) the "gaps question", because it has to do with the existence of "gaps" or "anomalies" in current evolutionary theory's ability to explain/predict known phenomena. Sometimes, (Q3) gets run together with other epistemological questions, such as:

(**Q4**) Is there (*any*) scientific evidence out there that *favors* ID *over* ET?

This question involves the existence of evidence that allows us to favorably *epistemically contrast* ID with ET as an *alternative account* of *some* phenomena. As we will see below, even if the answer to question (Q3) is "YES", the answer to (Q4) may well be "NO". That is, *even if* there are some phenomena ET does not adequately explain or predict, it may also be the case that ID does *no better* on this score (given traditional standards for judging the quality of scientific explanations). Finally, one must also be careful not conflate either (Q3) or (Q4) with:

(**Q5**) Given our *total* evidence, should we *reject* ET in favor of ID?

Rejection is a rather strong attitude to take toward a scientific theory. In the next section, we will see that scientists usually require far more than just *some* evidence against their best theory (and in favor of an alternative theory) before they are willing to give up on an existing scientific paradigm (in favor of a new paradigm). As such, question (Q5) may seem a little vulgar (and even beside the point). But, as we will see in later sections, some contemporary ID-theorists seem to suggest that the rejection of ET (in favor of ID) is imminent.

Because I am personally most interested the epistemological aspects of the ID controversy, I will ultimately spend the most time talking about questions (Q3)–(Q5). My discussion of these questions will trade largely on analogical considerations raised by thinking about a well-known case from the history of modern physics, to which I now turn.

2. A Canonical Example from the History of Modern Physics

Whenever we have competing scientific theories T1 and T2 concerning some empirical domain of application, we may ask questions analogous to (Q3)–(Q5) about T1 and T2. To fix our ideas, let's begin by talking about the following well-known competing historical scientific theories:

(**T1**) Newtonian celestial mechanics (*i.e.*, Newton's theory of celestial motion).

(**T2**) Einsteinian celestial mechanics (*i.e.*, Einstein's general-relativistic theory).

Our story about T1 and T2 begins in 1900. At that time, there was an intriguing piece of evidence, which had long been accepted by the scientific community. That bit of evidence was:

(**E**) The orbit of Mercury precesses, and the magnitude of its precession is *p*.
[The precise numerical value of the magnitude of the precession of Mercury's orbit is not important for our present purposes, so I will just call this value "*p*".]

This evidence was intriguing to the scientific community in 1900 because it appeared that the prevailing theory of celestial motions (T1) was unable to adequately predict or explain (E). Indeed, Newton's theory seemed to predict a value (*p**) for the precession of Mercury's orbit that was *significantly different than the observed value p*. In other words, the analogue of question (Q3) would have been answered "YES" by (the majority of[2]) the scientific community in 1900, concerning Newton's theory of celestial motions, because there *was* existing evidence (E) that Newton's theory seemed unable to adequately explain or predict. In other words, the precession of Mercury's orbit (E) constituted a *gap* (or *anomaly*) for Newton's theory (T1) in 1900. What about the analogues of questions (Q4) and (Q5), as applied to T1 and T2? Let's take (Q4) first. In 1900, several years before Einstein's theory T2 had been precisely formulated, there was no known (scientific) *alternative* to Newton's theory that did a *better* job of predicting (E). As such, there was no known (scientific) theory in 1900 that this evidence (E) *favored over* Newton's theory (T1). That all changed in 1915, when Einstein showed that his general relativistic theory (T2) of celestial motion *does* accurately predict (and adequately explain) the Mercury evidence (E). As such, in 1915, the analogue of question (Q4) concerning Newton (T1) *vs* Einstein (T2) would have been answered "YES", since the evidence E *favors* T2 *over* T1.

What about the analogue of question (Q5) concerning our historical example? *Just one* failed prediction is usually not sufficient to force the *rejection* of such a well-entrenched scientific theory. That is, it typically takes a rather *significant number and variety* of "gaps" or "anomalies" before the scientific community comes to believe that a theory is *false* (*i.e.*, to *reject* the theory). But, during the early part of the 20th century, more and more "gaps" or "anomalies" of Newtonian theory were discovered, and eventually Newton's theory came to be viewed as

[2] There were some "die hard" Newtonians who reacted differently to the perihelion data (at first). Such "die hards" tinkered with the background (auxiliary) assumptions (*e.g.*, by postulating the existence of "unobserved planets," *etc.*) so as to get Newton's theory to make a more accurate prediction about E. But, eventually, these desperate attempts to save Newtonian theory failed, and even the "die hards" had to concede that Newtonian theory simply made a less accurate prediction than T2 in this case. To simplify my story, I am not worrying about such "die hards". This is reasonable for my present purposes, since nowadays *nobody* thinks Newton's T1 can be saved. See (Roseveare 1982) for an in-depth and nuanced study of the history of the problem of explaining/predicting E.

false by the scientific community. Two crucial observations about this "rejection" of Newton's theory are in order at this point. First, the existence of a *superior alternative* (T2) to Newton's theory — *for which no known "gaps" or "anomalies" existed* — was an important part of the backdrop of the "rejection" of Newton's theory in the early part of the 20th century. If no better alternative scientific theory had been discovered, it is unclear whether Newton's theory would have been (or should have been) confidently rejected by the scientific community. Second, even though most scientists (by, say, 1950) would have come to believe that Newton's theory (T1) is *false*, they would have *also* believed that T1 was *very close to the truth, within many domains of application*. Indeed, to this day (nearly 100 years on), NASA continues to use Newtonian theory to predict the paths and orbits of spacecraft and other celestial bodies. So, while Newton's theory is (we would now say) *false*, it is still *very accurate*, and also *very useful for predicting and explaining many phenomena*. Perhaps this has something to do with why *Newtonian theory is still the theory that is taught in our public school science classes*! I'll return to that point later. But, let's not get ahead of ourselves. For now, let's wrap-up our discussion of the T1/T2 controversy by stating some lessons we should take away from this historical example. I think most scientists and philosophers of science nowadays would accept the following four claims about (or "lessons learned" from) the T1/T2 controversy:

1. E favors T2 over T1. That is, the Mercury perihelion data favor Einstein's theory over Newton's. [More precisely, this implies (*at least*) that T2 *confers a greater probability on* E than T1 does. More on this probabilistic precisification of "favoring", below.]
2. T1 is false, but very accurate and explanatory (in some domains). That is, Newton's theory is still the theory of choice in some domains of application. [Specifically, it is still tops among high school physics teachers! More on that sociological point, below.]
3. T2 is (strictly speaking) *more* accurate and more explanatory (overall) than T1. In part, this is because T2 even explains why T1 is as accurate as it is in various domains of application, since T1 can be seen as a "limiting special case" of T2. For low speed, low energy situations, Newtonian theory is adequate by almost any standard of accuracy or explanatoriness, and Eisntein's theory explains why *that* is the case.
4. It is reasonable to "reject" T1 (in favor of T2!) because:
    a. There are *sufficiently many* "gaps" or "anomalies" that T1 cannot explain/predict.

b. We have an *alternative* scientific theory T2 that: (*i*) is *more accurate and explanatory than* T1, (*ii*) explains why T1 is as accurate as it is (when/where it is), and (*iii*) does not (yet!) have a significant number of "gaps" or "anomalies".

I'd also conjecture that most scientists and philosophers of science would view (1)–(4) as *typical* of such episodes in the history of science. The way science evolves is (typically or often) by the development of new, more predictively accurate and explanatory theories which *supercede* old theories. What happened with Einstein and Newton had happened before with Newton and Kepler, and before that with Kepler and Copernicus, *etc*. When Newton's theory superceded Kepler's, new evidence had surfaced that favored Newton over Kepler, and eventually enough such "amomalies" were discovered until Kepler's theory was "rejected" and Newton's "accepted". Moreover, Newton's theory explains why Kepler's theory is as accurate as it is in its intended domain of application. Nonetheless, later on, Newton's theory was *itself* "rejected" in favor of Einstein's *still better* theory. And, since then, Einstein's theory has been "accepted" by the scientific community. However, Einstein's theory may *itself* be superceded someday (in fact, I bet many physicists hope their favorite new theory will be the one to do it!). And, if it is, this will not be the end of science, but only the beginning of a new scientific paradigm. In light of these considerations, I think we should add a fifth and final point (or "lesson learned"):

5. T2 is the best theory we now have (in its domain), and so we now "accept" it. But, this "acceptance" is defeasible, and could turn into "rejection" if an *even better* theory T3 were to come along (then T2 would play a role analogous to T1's old role).

These five points (1)–(5) about the Einstein/Newton "controversy" in 20th century physical science will prove useful for historically and philosophically framing our discussion of the epistemological questions (Q3)–(Q5) concerning the ID/ET controversy in contemporary biological science. By now, you can probably see where this is going. I'm going to try to motivate several philosophical lessons about ID *vs* ET by comparison with the historical case of Einstein *vs* Newton. With this in mind, I am now going to move on to questions (Q3)–(Q5). Our discussion of (Q3)–(Q5) will then lead us naturally back through the demarcation question (Q2). And, once we've tackled (Q2)–(Q5), we'll be ready to say a few (modest, but hopefully thought provoking) things about (Q1), which is the question in all the papers nowadays.

3. The Epistemological Questions (Q3)–(Q5) and the Demarcation Question (Q2)

Using the Einstein/Newton case as a historico-philosophical model, we can now begin to intelligently address our epistemological questions (Q3)–(Q5). Let's start with (Q3). Are there any biological phenomena that our current (best) evolutionary science is at a loss to explain and/or predict? Proponents of ID certainly seem to think so. And, many biologists disagree. Anyhow, there is a rapidly growing literature on this, and I will not try to survey it here.[3] But, to summarize, the basic worry IDers seem to have is that certain features or patterns in the biological domain (call such features E*) are (allegedly) not adequately explained or predicted by contemporary evolutionary theory. For the sake of argument, I will (for now) simply *grant* this assumption to the proponents of ID. That is, for now, I will *grant* that there is *some* evidence E* that is not adequately predicted and/or explained by ET. What follows from this assumption? Not much. Think back to Newton *vs* Einstein. In 1900, *everybody knew* that E was not adequately explained/predicted by Newton's theory. Was that sufficient grounds to "reject" Newton's theory? Certainly not. It wasn't until a *concrete, superior, alternative theory* was discovered that the Newtonian paradigm was *seriously* called into question. It is fair to say, however, that (by 1900) many scientists were growing increasingly concerned about this anomaly E. On the other hand, it is also fair to say that there was no outcry for the physics textbooks of the day to be revised with a caveat saying that Newtonian celestial mechanics was "just a theory" or "just one theory among various competing theories". Indeed, even *today's* high school physics textbooks teach Newtonian mechanical principles as if they are true, when the scientific consensus is that they are false (and this has been the case for nearly 100 years). I will return to these sociological issues later. Coming back to the issue at hand, it is now time to ask: Do proponents of ID have a *concrete, superior alternative* to ET (like Einstein's T2) in mind? They must, if this is going to be a *scientific* controversy that fits the mold of our canonical historical example. But, what could this *concrete, superior alternative* to ET *be*? This brings us back to our *demarcation question* (Q2). We must now think about what ID *is*.

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[3] For instance, Michael Behe is one (of very few) biological scientists who defends ID. His book (Behe 1996) discusses several (alleged) "anomalies" or "gaps" for ET. The textbook "Of Pandas and People" (Davis and Kenyon 1993) is another recent source of (alleged) "gaps". Some of these "gaps" are also discussed by (Brauer *et al.* 2005) in their encyclopedic survey paper. Various scientists and philosophers of science have responded to the recent "gap" allegations of ID-theorists (as an easy Google search will quickly reveal). See (Pennock 2001) and (Young and Edis 2004) for two recent surveys of responses to various charges that have been made by proponents of ID. Such challenges to ET are not new, of course. See Michael Ruse's book (Ruse 1982) for an extended, historical discussion of questions (Q3)–(Q5) and their ancestors in a long lineage of similar debates about evolution.

People who write about ID are not always clear about what it is and what it is not. For instance, sometimes ID is treated as having *creationist* implications, and sometimes it is not. In any case, what *all* forms of ID seem to have in common is the implication that there is some kind of "intelligent design" at work in the universe, which is (presumably) responsible (*at least*) for phenomena such as E* that are (allegedly) not adequately explained or predicted by purely naturalistic, evolutionary means (*viz.*, by contemporary evolutionary theory ET). To my mind, "intelligent design" suggests the existence of some *agent* who *intentionally* builds certain features or patterns into the "design plan" of (some aspect of) the universe (*e.g.*, E*). Evolutionary theory, on the other hand, does not *require* any such "design plan" for the universe, nor does it *require* the existence of an author for such a plan. Of course, ET is not necessarily *inconsistent* with the existence of such a "design plan" (or a designer) either. It is possible that there is a "design plan" for the universe *and* that this plan includes the operation of natural selection (and other natural, evolutionary forces) *exactly as characterized by* ET. On this reading, ID it is not *in competition with* ET at all, since on this scenario "intelligent design" can only operate in the biological domain *through the processes characterized by* ET.[4] This would make ID *dis*analogous to Einstein's theory, which is *incompatible* with Newton's theory (*i.e.*, T1 and T2 *cannot* both be true). And that would suggest that ID — so understood — is not an *alternative* to ET in the requisite sense. As such, *this* can't be the proper understanding of ID, if it is going to play a role analogous to Einstein's *alternative* to Newtonian cosmology. It therefore seems that we need a reading of ID according to which the explanation/prediction of features E* does *not* involve *only* the naturalistic, evolutionary factors characterized by ET. This seems to imply that we need ID to posit some sort of "intelligent intervention" into biological systems that transcends the kinds of processes systematized by ET. The only way that I (personally) can make sense of all of this is to say that ID — in order to be a bona fide *alternative* to ET — must postulate *some* sort of "super-evolutionary"[5] *intentional action* on the

[4] The idea that God uses the evolutionary process to produce what we observe is now called "theistic evolutionism." This is a form of what in the 18th century was called "Deism." Many mainstream churches, *e.g.*, the Catholic Church, embrace theistic evolutionism. See (Miller 2000) for a recent discussion of theistic evolutionism.

[5] While some actions of ID's author must be "super-*evolutionary*" (if ID is going to be an *alternative* to ET), there is still a question as to whether the ID-author is herself is "super-*natural*". Contemporary IDers often want to deny that ID *implies* that the author of the "design plan" is "super-natural". But, in fact, most modern IDers *themselves believe* that the author *is* "super-natural". For this reason, I will assume that the author is "super-natural." In any event, we've been told very little about the author, and that is what makes things difficult.

part of *some* agent[6] as part of its explanation for features like E* that (allegedly) cannot be adequately explained by ET. This naturally raises questions like: "What is this agent like?", "What are its intentions?", "What is its 'design plan' for the salient parts of the universe?", *etc.* When one talks about *design*, one should say *something* about the actual nature and intentions of the design*er* (or, at least, something about the actual nature of the "intelligent design *plan*") — *if* one is going to *use* "intelligent design" to generate scientific predictions and explanations of actual phenomena (*e.g.*, E*). This is not a new problem, of course. This problematic feature of arguments concerning "design" has been discussed for many centuries. For an excellent recent survey, see (Sober 2004). Descartes (1984, p. 258) — who assumes that *God* is the "designer behind intelligent design" — eloquently articulates the central problem here as follows:

> In ethics…where we may often legitimately employ conjectures, it may admittedly be pious on occasion to try to guess what purpose God may have had for the direction of the universe; but in physics, where everything must be backed up by the strongest arguments, such conjectures are futile. We cannot pretend that some of God's purposes are more out in the open than others; they all are equally hidden in the inscrutable abyss of his wisdom.

If Descartes is right (and I suspect that he is), then an ID theorist who takes God to be the "intelligent designer" (and, let's face it, most of the contemporary defenders of ID *do in fact* fall into this category) certainly has his work cut out for him. More generally, without *some* concrete idea of the purposes behind "intelligent design", it is difficult to see how we could ever *know* that ID *really does* provide a *superior, alternative explanation* of *any* biological phenomena (E*). And, without such knowledge, how can we ever see ID as a theory that has the potential to *supercede* our current Darwinian paradigm? Keep in mind that, in order to supercede ET, a theory would not only have to be more predictively accurate and explanatory than ET concerning problematical bodies of scientific evidence such as E* (if there be such), it should also be able to shed some light on why ET has been as successful as it has in its intended domain. If the success

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[6] It is sometimes denied that ID *implies even the existence* of a design*er*. But, I have never understood precisely what this denial is supposed to amount to. After all, ET-theorists are happy to talk about *teleology* — they are not uncomfortable to "purposefulness" in nature, unless this "purposefulness" manifests itself in a way that *transcends* the naturalistic processes described by contemporary evolutionary theory (Sober 2000, §3.7). So, ID — if it is to be an *alternative* to ET — cannot *merely* imply the existence of "purposefulness" in nature. Moreover, as I mentioned in the previous footnote, most defenders of ID *do* believe in the existence of a ("super-natural") author of the "intelligent design plan", so this denial is rather disingenuous. See, for instance, see footnote 8 of (Fitelson *et al.* 1999) for a concrete example of an intelligent design theorist who has said incongruous things about the implications of ID concerning the existence of a designer (and the "super-natural" status of said designer).

of ET is *utterly mysterious* from the point of view of ID, then ID can hardly be seen as a theory which is capable of *superceding* ET.

Many historical proponents of the "design argument" — most notably, William Paley — have been quite sensitive to this problem, and they chose to tackle it *head-on*. Paley *directly* addressed this issue, and he argued (by presupposing various things about the nature of God[7]) that God's purposes were such that he was (more) likely to "design" a world containing features like E* that (allegedly) ET alone cannot explain. It is unfortunate (but not that surprising, I think) that contemporary ID-theorists have *not* taken this challenge as seriously as Paley did. If IDers were to take the traditional Paley line, then it would become rather clear that theirs is a theory which relies (at least in part) on *religious* doctrine to explain and predict *scientific* data.[8] And, I doubt this strategy would effectively further their current political and social ends. I'll return to our central, socio-political question (Q1) later. For now, I want to say something about how contemporary ID-theorists *have* reacted to this "Cartesian challenge". Interestingly, but again perhaps not surprisingly, they have *not* responded by trying to construct a "scientific theory of action/mind for the 'designer'" (or even a precise story about what the "design plan" for the universe might look like). Instead, contemporary IDers have responded by trying to argue that we should reject the traditional epistemological standards of science. Traditionally, scientific inference has been a *contrastive* enterprise in which *competing* scientific theories are *tested against each other*, using certain bodies of scientific data. In the case of Einstein and Newton, we had evidence E which was *better accounted for* by T2 than T1. And, "by better accounted for", I have *at least*[9] the following precise claim in mind: *Einstein's theory confers a*

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[7] This is somewhat anachronistic. Paley's argumentative strategy was actually more subtle than this over-simplified description suggests. Paley *first* tries to argue for God's *existence*, and he tries to do this *without* assuming anything (specific) about God's attributes. Then, once he thinks he has *established* God's existence, he goes on to ascertain God's (specific) attributes. Be that as it may, in order to estimate likelihoods such as Pr(E* | ID), it seems to me that Paley *must* presuppose *something* about the nature of God. It is in this sense that I think Paley was (perhaps implicitly) presupposing *something* about God's nature, even in the first stage of his reasoning about Design. I just don't see how it is *possible* to estimate Pr(E* | ID) without assuming *something* (although perhaps nothing that specific) about the nature of the author of the "design plan". See, also, Sober (2004).

[8] I presuppose here that assumptions about the nature of God (or any "super-natural" agent) are *not independently testable* by any known scientific method. We cannot just *invent* auxiliary assumptions about the author's purposes here. We must have some *independent justification* for such assumptions, and one that would be acceptable to *competing scientists*. I don't have the space to argue for this presupposition here. Various compelling arguments for this presupposition can be found in (Sober 1999/2004), (Kitcher 1984), and (Pennock 1998).

[9] In (Fitelson 2005), I argue that a proper probabilistic account of "E favors T2 over T1" requires sensitivity not only to the *likelihoods* Pr(E | T1) and Pr(E | T2), but also to the *catch-alls* Pr(E | *not*-T1) and Pr(E | *not*-T2). So, in general, even establishing Pr(E | T1) < Pr(E | T2) is *not sufficient* for establishing that E favors T2 over T1. And, in addition to such *probabilistic* (favoring) relations, considerations of *explanatoriness*, *etc*. may *also* be important.

greater probability on the evidence E than Newton's theory does. That is, Pr(E | T1) < Pr(E | T2). In technical jargon, this inequality says that T2 has a *greater likelihood* than T1, *relative to the evidence* E. This kind of *contrastive evidential comparison* is a crucial part of traditional scientific methodology.[10] Descartes and Paley were sensitive to this. They realized the importance of being able to say *something* about the likelihood of "intelligent design": Pr(E* | ID). Contemporary ID-theorists, on the other hand, tend to argue that the traditional, contrastive scientific epistemology which requires such comparisons is *itself* flawed. Several years ago, William Dembski (one of the leading contemporary IDers) wrote a book which pushes this kind of "new scientific epistemology".[11] In that book, Dembski defends a *non*-contrastive epistemology in which the only salient probabilistic quantity we need to consider in the debate between ET and ID is Pr(E* | ET). According to Dembski, if Pr(E* | ET) is *sufficiently low*, then (other things being equal) *that — by itself —* is sufficient grounds for "rejecting" ET. As we have already seen, this runs counter to scientific practice, which refuses to "reject" a prevailing paradigm until a concrete, alternative scientific theory is proposed which *better* accounts for the evidence E* in question. This, at the very least, would require an argument for the *likelihood comparison*: Pr(E* | ET) < Pr(E* | ID). It may well be (and I am simply *granting arguendo*) that there are phenomena E* out there that would be very improbable if evolutionary theory were correct. But, unless we have good scientific reasons to think that there is an *alternative* theory which *explains* E* *better* (at least, one that has a great*er* likelihood than ET, relative to E*), this would not — by the standards of traditional scientific epistemology — be sufficient to "reject" ET. Thus, while Paley chose to rely theological presuppositions in an attempt to motivate the *contrastive likelihood comparison* Pr(E* | ET) < Pr(E* | ID), contemporary ID-theorists abandon traditional contrastive scientific epistemology altogether, in favor of a non-contrastive approach that sanctions "rejecting" theories that are sufficiently unlikely, *even if no superior alternative theory is known*. Keep in mind that, for all we know, it could turn out that Pr(E* | ID) is *even*

[10] See (Sober 1990), (Royall 1997), and (Fitelson 2005) for extended discussions of the inherently contrastive nature of scientific epistemology (*i.e.*, the proper interpretation of statistical scientific evidence).

[11] Here, I have in mind (Dembski 1998). Elliott Sober, Chris Stevens and I wrote an extended and very careful review of Dembski's book soon after it came out (Fitelson *et al*. 1999). Unfortunately, our review — which presents many serious challenges for Dembski's proposals concerning ID — is rarely even mentioned nowadays in connection with Dembski [the recent scholarly survey (Brauer *et al*. 2005) is a notable exception]. Presently, I have only scratched the surface of the deep epistemological problems with Dembski's approach. I recommend reading our review to get a clearer picture of what he is up to. Alvin Plantinga makes some similar non-contrastive epistemological maneuvers in (Plantinga 1993). See (Fitelson & Sober 1998) for a detailed critique.

lower than Pr(E* | ET). Indeed, for all we know, it could turn out that Pr(E* | ET) is greater than Pr(E* | T) for *any* alternative scientific theory T that we might discover in the future. Contemporary ID-theorists don't seem to be bothered by such possibilities. But, it is precisely because of such possibilities that traditional scientific epistemology *is* (and *should be*) *contrastive*. To summarize, the ID-theorist seems to face the following "Cartesian dilemma":

- Accept (as Paley did) the traditional contrastive scientific epistemological requirement of arguing that Pr(E* | ET) < Pr(E* | ID). In this case, the Cartesian worries about religious speculation on the "purposes of the intelligent designer" loom large, and we seem to get pushed toward something that more closely resembles *theology* than science.

OR

- Abandon (as Dembski does) the traditional contrastive scientific epistemological requirement of arguing that Pr(E* | ET) < Pr(E* | ID), and instead try to motivate a *non*-contrastive scientific epistemology which doesn't require such comparisons. In this case, we seem to get pushed toward something that more closely resembles *epistemology* than science. Moreover, the epistemology that one must defend on this horn is not an attractive one, because it is *unreasonable* (not merely "non-traditional") for a scientist to "reject" a paradigm in the absence of a concrete superior alternative to it (and without *lots* of evidence that speaks against their best theory and in favor of said alternative).[12]

These considerations indicate that ID-theorists have some serious work to do if they aspire to create a new paradigm for biological science (as Einstein did for physical science).

At this juncture, I would like to summarize and highlight two striking *dis*analogies between the canonical scientific controversy in the early 20th century concerning Einstenian *vs* Newtonian cosmology and the current controversy between ID and ET.

First, not only were there "gaps" or "anomalies", which Newtonian cosmology could not explain, but there was a *concrete, alternative scientific theory* that (clearly, and by pre-existing scientific standards) accounted for these phenomena *better than* Newtonian theory did. By this,

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[12] If scientific theories — *by themselves — deductively entailed* their predictions *and* we could be *absolutely certain* on the basis of scientific experimentation and observation that evidence reports such as E* were true, then "rejection" might be a reasonable attitude toward a theory that "makes a single false prediction" — *even in the absence of an alternative*. However, scientific theories do *not* (by themselves) deductively entail their predictions for two reasons: (1) they require *auxiliary assumptions* in order to render predictions (this is the *Duhem–Quine* thesis – see Sober 1999 and *fn*. 8 above), and (2) even in conjunction with the requisite auxiliaries, all we get from scientific theories are *likelihoods* of data *not logical guarantees*. Moreover, our observations are *not* infallible. For these reasons, "naive *modus tollens*" is not a canon of contemporary scientific inference. See (Sober 2002).

we mean *at least* that Pr(E | T1) < Pr(E | T2). Moreover, Einstein's theory is able to explain why Newton's theory had been as successful as it had. Unfortunately, contemporary ID-theorists like Dembski are unwilling to explicitly argue (as Paley did) that their "design theory alternative to ET" provides a *better explanation* of (or even that it *better predicts*) E* than ET does, or that ID can shed any light on the impressive *successes* of ET. In the absence of such arguments, I don't see how we have any hope of generating any compelling reasons to "reject" ET in favor of ID.

Second, if no superior alternative to Newtonian celestial mechanics had been discovered, it would (I presume) have been unreasonable to "reject" Newtonian theory in favor of an "intelligent design cosmology" (*i.e.*, a cosmological analogue of ID).[13] And, yet, ID-theorists like Dembski are inviting us to "reject" ET in favor of ID on the basis of E*, merely because ET (allegedly) does a "sufficiently poor" job of explaining/predicting E*, and *independently* of whether ID does *any better* on that score. Had Dembski been alive in 1900, would he have said something similar about Newtonian theory and the Mercury perihelion data E? That is, would he have suggested that we should reject Newtonian cosmology in favor of an "ID-cosmology"? Indeed, would he have even recommended that physics textbooks of the day be altered so as to caution students that Newtonian mechanics is "just a theory" or "just one among many theories" (with the analogue of ID being a "legitimate alternative" to Newtonian mechanics)? I think not. After all, once Einstein's theory came along in 1915, nobody would have seen the need for such an "ID cosmology" any longer, and they would have simply accepted Einsteinian theory (as they in fact did). Moreover, even though scientists *have* accepted Einstein's theory (and rejected Newton's), our physics textbooks *still* don't come with any caveats about Newtonian theory being "just a theory". In fact, Newtonian mechanics is *still* taught in our high school physics classes *as it if were true*! So, if such moves wouldn't have made sense (even to Dembski *et al.*) in 1900 in the context of physical science, then why should such moves make sense *now* in the context of biological science? As Descartes' remark suggests, the phenomena in question are all *ultimately* within the domain of physics anyway, so how can such an asymmetry be reasonably

[13] It is interesting to note that Newton *himself* was not afraid to appeal to divine intervention to explain *some* phenomena, which he considered to be outside the proper domain of application of his theory, *e.g.*, why the planets all orbit in the same plane and in the same direction. But, the magnitude of the precession of the orbit of Mercury is *within* the proper domain of Newton's theory. And, besides, Einstein's theory T2 *naturalistically* explains E, so it seems superfluous (at best) to appeal to divine intervention for the purposes of explaining E. See (Odom 1966) for a nice discussion of the interplay between celestial mechanics and religion in the Enlightenment.

sustained? This second point of disanalogy has special relevance, I think, to our main socio-political question (Q1), to which I (finally) now turn.

4. The Socio-Political Question (Q1)

Should ID be part of our public school curriculum? This is a very complex socio-political question, and I do not claim to be expert enough on the salient legal, ethical, and sociological issues to render a judgment. However, I would like to say something (modest) about this issue. I will begin by focusing on the more precise questions (Q1.1) and (Q1.2) I stated above:

(**Q1.1**) Should ID be included in the *science* curriculum of our public schools?[14]

(**Q1.2**) Should ID be included in *some* part of our public school curriculum?

Let me address (Q1.1) first. As I noted at the end of the last section, Newtonian celestial mechanics is part of our public school science curriculum, but Einsteinian General Relativistic celestial mechanics is *not*. This is *despite* the fact that both theories are clearly scientific (if anything is), and *despite* the fact that *Einstein's theory superceded Newton's nearly a century ago*! This suggests that *even if* a theory is *our best current scientific theory* in some domain, it does *not* follow that this theory would (or should) be made part of our public school science curriculum. Thus, *even if* we were willing to *grant* the ID-theorist that their theory is (not only *a* scientific theory, but) a *better scientific theory than* ET, that *by itself* would not (and should not) earn ID a place in our children's science textbooks.[15] Decisions about which scientific theories to include in our public school science curricula are clearly much more subtle and complicated than our recent obsession with the "demarcation" question (and even the epistemological questions!) might suggest. I take it that we want our public schools to prepare *some* of our students for deeper and more sophisticated scientific studies, but we also want our schools to give *most* students a *basic* understanding of *some* of the most successful explanatory and predictive schemes of modern science. While it would (perhaps) be nice (in a perfect world) to

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[14] I actually have a more specific question in mind here, which is whether ID should be taught *as a scientific theory* in some public school science course(s). We can imagine ID being discussed in the context of a biology course (say, in a *historical* module of a biology course which covers various controversies that have cropped-up in the development of modern biological theory), but not *as* a scientific theory. Here, I will assume that being "included in the public school science curriculum" also entails being taught *as a scientific theory*.

[15] Not even the staunchest of IDers is suggesting that ID should be taught in public school biology courses *at the exclusion of* ET. Ironically (and disanalogously), however, in the case of scientific theories of celestial mechanics, not only is the scientifically inferior physical theory being taught in our public schools, it is being taught *at the exclusion of* the superior physical theory! So, even if (*per* highly improbable) ID turns out to be a superior biological theory to ET, it may be that ET should *still* be taught in our public schools — and *at the exclusion of* ID!

teach our students the best known scientific theories, it may (practically) not be worth the cost. That is, it may serve the public interest *better* to teach theories that are not (strictly speaking) the best known theories, but which are accurate and explanatory *enough* to undergird an acceptable science education for the "average American". Whatever aims we have in designing our public school science curricula, I think we would all agree with the decision to include Newtonian celestial mechanics (but *not* Einsteinian celestial mechanics!) in our science curricula, despite the fact that Newtonian theory is widely thought to be *false and inferior to* the Einsteinian alternative. It is probably just too costly and difficult to teach Einsteinian science (generally) to our public school students. And, besides, Einsteinian physics is *almost never used or needed in real life situations* (remember, NASA doesn't even use it!). So, it is unclear whether there would be a significant benefit to teaching Eisnteinian theory in our public schools. Now, the question at hand is whether we would also all agree with a decision to include ID in our public school science curriculum. This is far less clear. First, it is far less clear whether ID is a *scientific* theory in the first place (I take it there is no unclarity about the scientific status of Einsteinian cosmology). There has been a tremendous amount of debate on this "demarcation question" recently, especially in connection with the question at hand (Brauer *et al*. 2005). I don't want to say too much about that issue here. But, our simple "Cartesian dilemma" above does seem to suggest that ID (at least, the contemporary "Dembskian" flavor of it) is either partly a theological theory or partly a philosophical (*i.e.*, epistemological) theory. Be that as it may, perhaps the more important lesson we can learn from our historical example about Einstein and Newton is that *even if* ID *is* a scientific theory — *even* one that that ultimately *supercedes* ET — this would not (by itself) be sufficient to warrant including ID in our science curriculum. Justifying *that* will require a far more careful consideration of *both* the nature of biological science *and* the aims of our public education system.[16] Given what we've been told so far about the quality and utility of ID as a scientific theory (which is almost nothing), I can't see any compelling reason to

[16] My line of argument here may seem to *too* powerful now, since it may appear that it could be turned around and used to justify teaching *ID* even if *ID* is false. What my argument has shown is that it is sometimes appropriate to teach our students false theories (like Newtonian mechanics). This does *not* imply that we have *carte blanche* to teach whatever false theories we might cook up. Newtonian mechanics is a highly predictively accurate and explanatory theory (by almost any standard), which is therefore (still) of great value both practically and theoretically. Moreover, it can be seen as part of a lineage of scientific theories, each superceding the previous theory by improving on it, and by shedding some light on why the previous theory was as accurate as it was in its domain. Such non-trivial standards of quality and utility should be met by the scientific theories we choose to teach.

include ID as part of the public school science curriculum. It seems premature (at best) to place figures like Dembski and Behe alongside Darwin in our public school biology textbooks.[17]

What about (Q1.2)? Should ID be included in *some part* of our public school curriculum? This question seems to me to be far less contentious, but no less interesting. In fact, it raises some important issues concerning the current state of our public education system. In many other industrialized countries (*e.g.*, France) philosophy courses are a rather routine part of public education. This is (generally) not the case in the United States. There are certainly some public schools in the U.S. that offer *electives* with some philosophical content, but philosophy courses are not a *standard* part of any public school curricula with which I am familiar. I, for one, would be strongly in favor of expanding our public school curricula to include a healthy dose (*i.e.*, a *modicum*!) of philosophical content. This could involve (say) a survey course (perhaps for high school seniors) that covered some basic logic, critical thinking, philosophy of science, epistemology, metaphysics, ethics, *etc*. In the context of such a philosophy course, it would (I submit) be perfectly reasonable and appropriate (even constructive!) to discuss the ID/ET controversy, as well as other controversies in (*e.g.*) the history of science. It seems that there are many American parents who would like their children to ponder and discuss these sorts of issues in their public school classrooms. One potential benefit of this kind of "philosophical strategy" is that it might provide a way to include topics such as the ID controversy, which don't seem to fit neatly (or constitutionally!) anywhere in our existing public school curriculum. To my mind, a more important (and general) potential benefit of adding such a philosophy course to our public school curriculum is that this just might increase the amount of *critical thinking* that goes on in our public schools. And, that, I think we can all agree, would be a *good thing*.

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[17] Here, I am in agreement with Judge John E. Jones III who was the presiding judge in the now infamous case of *Kitzmiller et al. v. Dover Area School District et al.* His trenchant 139 page ruling of December 20, 2005 (which can be readily downloaded from various sources on the internet) makes this clear. Of course, the *legal* rendition of question (Q1) is of a significantly different nature than the *philosophical* rendition of (Q1) that I have in mind in this paper. If a theory is deemed to be "religious" (in a certain sense), then this would disqualify it (on *constitutional* grounds) from being included in our public school curriculum. The legal criterion now in place for determining whether ID is "religious" concerns the *purposes* people have for promoting it (*i.e.*, is the primary *goal* of IDers to *advance religion*?). This makes our demarcation question (and our epistemological questions) only *tangentially* relevant (*at best*). From a legal point of view, such questions are probative only insofar as they help us make reasonable inferences about the *socio-political aims* of contemporary IDers (Brauer *et al.* 2005). In this paper, I am not concerned with the IDers' socio-political aims. I am interested in *philosophical* questions like those I have been discussing. On that score, I think the *contemporary* ID-theory is better viewed as (at least partly) a *philosophical* (*viz.*, *epistemological*) doctrine, as opposed to a *religious* one. Moreover, in light of the lessons learned from the historical case involving Einstein and Newton, *even if* ID could manage to avoid our "Cartesian dilemma" and thus qualify as a properly *scientific* theory, that (alone) would not settle the philosophical question (Q1) that we are considering. These considerations are independent of the *socio-political aims* of IDers.

References

Behe, M.J., (1996), *Darwin's Black Box*: *The Biochemical Challenge to Evolution*, New York: *The Free Press*.

Brauer, M., Forrest, B., and Gey, S., (2005), "Is it Science Yet?: Intelligent Design Creationism and the Constitution," *Washington University Law Quarterly* **83**(1): 1–149.

Davis, P. and Kenyon, D.H., (1993), *Of Pandas of People*, Dallas: *Haughton*.

Dembski, W., (1998), *The Design Inference: Eliminating Chance through Small Probabilities*, *Cambridge University Press*.

Descartes, R., (1984), *The Philosophical Writings of Descartes* (Volume 2), *ed.* and trans. J. Cottingham, R., Stoothoff, D. Murdoch, and A. Kenny, Cambridge: *Cambridge University Press*.

Fitelson, B. and Sober, E., (1998), "Plantinga's Probability Arguments Against Evolutionary Naturalism," *Pacific Philosophical Quarterly* **79:** 115–29. URL: http://fitelson.org/plant.pdf.

Fitelson, B., Stephens, C. and Sober, E., (1999), "How Not to Detect Design: A Review of William Dembski's *The Design Inference*," *Philosophy of Science* **66**: 472–88.
URL: http://fitelson.org/dembski.pdf.

Fitelson, B., (2005), "Likelihoodism, Bayesianism, and Relational Confirmation," *Synthese* (to appear). URL: http://fitelson.org/synthese.pdf.

Kitcher, P., (1984), *Abusing Science: The Case Against Creationism*, Cambridge, Mass.: *MIT Press*.

Miller, K. (2000), *Finding Darwin's God: A Scientist's Search for Common Ground Between God and Evolution*, New York: Harper Perennial.

Odom, H., (1966), "The Estrangement of Celestial Mechanics and Religion," *Journal of the History of Ideas*, **27**: 533–548.

Pennock, R., (1998), *Tower of Babel: The Case Against the New Creationism*, Cambridge, Mass.: *MIT Press*.

Pennock, R. (*ed*.), (2001), *Intelligent Design Creationism and Its Critics: Philosophical, Theological, and Scientific Perspectives*, Cambridge, Mass.: *MIT Press*.

Plantinga, A., (1993), *Warrant and Proper Function*, New York: *Oxford University Press*.

Roseveare, N. T., (1982), *Mercury's Perihelion from Le Verrier to Einstein*, New York: *Oxford University Press*.

Royall, R., (1997), *Statistical Evidence: A Likelihood Paradigm*, New York: *Chapman & Hall*.

Ruse, M., (1982), *Darwinism Defended: A Guide to the Evolution Controversies*, New York: *Addison Wesley*.

Sober, E., (1990), "Contrastive Empiricism," In Wade Savage (*ed*.), *Minnesota Studies in the Philosophy of Science (vol. 14)*, University of Minnesota Press, pp. 392-412.

Sober, E., (1999), "Testability," Presidential Address, American Philosophical Association Central Division. *Proceedings and Addresses of the APA*, **73**: 47-76.

Sober, E., (2000), *Philosophy of Biology* (Second Edition), Boulder: *Westview Press*.

Sober, E. (2002), "Intelligent Design and Probability Reasoning," *International Journal for the Philosophy of Religion*, 2002, *52*: 65–80.

Sober, E., (2004), "The Design Argument," In W. Dembski, A. Menuge, and M. Ruse (*eds*.), *Debating Design – from Darwin to DNA. Cambridge University Press*, pp. 98-129.

Young, F. and Edis, T. (*eds*.), (2004), *Why Intelligent Design Fails: A Scientific Critique of the New Creationism*, New Brunswick: *Rutgers University Press*.